2010-06

The Impact of the High School Junior ROTC Program: Does Treatment Timing and Intensity Matter?

Pema, Elda; Mehay, Stephen

Routledge

Pema, Elda, and Stephen Mehay, "The Impact of the High School Junior ROTC Program: Does Treatment Timing and Intensity Matter?," Defense and Peace Economics, v.21, no.3 (2010), pp. 229-247
http://hdl.handle.net/10945/56706

Defence and Peace Economics, 2010
Vol. 21(3), June, pp. 229–247

Routledge
Taylor & Francis Group

THE IMPACT OF THE HIGH SCHOOL JUNIOR ROTC PROGRAM: DOES TREATMENT TIMING AND INTENSITY MATTER?

ELDA PEMA† AND STEPHEN MEHAY

Graduate School of Business and Public Policy, Naval Postgraduate School, Monterey, CA, USA

(Received in final form 26 November 2008)

The Junior Reserve Officers' Training Corps is a high school program that combines classroom teaching with extracurricular activities. The program is located primarily in inner city schools and serves at-risk students. Its goals are multidimensional and include military preparation and improving academic achievement. Using High School and Beyond data we find that the program's effects depend on the timing and intensity of involvement. Test scores, graduation rates, and enlistments are higher for students who participate early in high school and for those who persist in the program. Conversely, we find few effects for students participating in the last two years of high school.

Keywords: JROTC; High school achievement; At-risk youth; Enlistments

JEL Classification: I2

INTRODUCTION

The Junior Reserve Officers' Training Corps (JROTC) is a high school program funded jointly by local school districts and the Department of Defense (DOD). Combined federal and local spending on the program is estimated to be roughly $600 million annually, which supports over 500,000 students in 3,400 high schools (US Department of Defense, 2008). Despite its size, numerous questions have been raised about the program's goals and orientation, and its impact on students. One recent article questions whether JROTC represents 'educational reform or militarization' (Berlowitz and Long, 2003), while a second questions whether JROTC instructors are 'role models or recruiters' (Atkins, 2005). Although these captions are meant to be provocative, they represent some of the policy issues surrounding the program.[1] Ironically, JROTC is attacked by observers outside of DOD for being a military recruitment program, while it is criticized within DOD for *not* being a recruitment program (Coumbe and Harford, 1996).

†Corresponding author. Graduate School of Business and Public Policy, Naval Postgraduate School, 555 Dyer Road, Monterey, CA 93943, USA. E-mail: epema@nps.edu

[1] Indicative of the controversy are anti-JROTC campaigns in some major cities (Nazario, 2007) and the decision in 2006 by the San Francisco School Board to eliminate JROTC from all city high schools (Tucker, 2006).

ISSN 1024-2694 print: ISSN 1476-8267 online
DOI: 10.1080/10242690903105554

Beyond the controversy over its military orientation, numerous features of JROTC make it of interest to public policy analysts. First, the program serves a diverse student population, which includes many at-risk and disadvantaged students. Nearly 40% of JROTC units are located at inner-city schools, about half of all enrollees are minorities, and over 40% are females. Second, the program is multidimensional – its goals include leadership, communication, physical fitness, citizenship, and improving graduation rates and non-cognitive skills, such as self-esteem (Crawford *et al*., 2004). Local school districts are attracted to the program by the potential for student gains in cognitive and non-cognitive skills, but also by the federal subsidy, which covers about 40% of program costs (Denver Public Schools, 1996). Finally, although a common perception is that JROTC's main goal is to stimulate interest in the military, most students (about 70% in our data) who enroll in JROTC some time during high school never enter the military.[2]

To date, few studies have evaluated JROTC. The most notable exception is a recent study which suggests that JROTC has a sorting effect that channels students into the military and away from post-secondary education (Pema and Mehay, 2009a). However, these results apply only to the average JROTC student enrolled in a typical JROTC school. Since most students who ever enroll in JROTC do not complete the 4-year curriculum, program, effects may differ for marginal students or those who participate for varying lengths of time or in different grades. Recent research on program evaluation questions the assumption that all treated individuals obtain the same average benefit (Blundell *et al*., 2004). Among studies reporting heterogeneous participation effects, Barron *et al*. (2000) find that the intensity of involvement in team sports affects students' post-school labor market success. Myers *et al*. (2004) also find that the duration of involvement in Upward Bound affects in-school outcomes.

This study supplements recent evidence on JROTC in Pema and Mehay (2009a) by investigating the impact of the timing and intensity of program involvement on student outcomes. We find that the program increases standardized test scores, especially for students who persist in the program. It also improves high school graduation rates for students who participate in the early grades. We find positive enlistment effects for the same two groups. In contrast, those who join JROTC in the later grades display no changes in graduation or enlistment rates. Finally, we find that school-level enlistments are higher in schools that host JROTC.

BACKGROUND

JROTC combines classroom instruction with extracurricular activities and community service. Students can join JROTC at any point in high school without incurring an obligation to enlist. Those who complete at least three years of JROTC and who do choose to enlist receive a higher pay grade upon entry into the military. Although JROTC is unique in many regards, it shares some elements with better-known high school initiatives, such as career academies and school-to-work programs, which offer mostly elective credit in vocational areas. Some JROTC activities are similar to standard extracurricular electives such as band and team sports.[3]

Prior research on JROTC has been limited and for the most part is descriptive in nature. One case study finds that in Chicago inner-city schools JROTC students performed no better in

[2] Surveys indicate that 30–50% of JROTC graduates intend to enlist (Bailey *et al*., 1992; Center for Strategic and International Studies, 1999). However, only a fraction of those with positive intentions actually enlist, and an even smaller percentage 'graduate' (i.e. complete the 4-year program).

[3] By way of comparison, JROTC (with 3300 schools) is nearly twice the size of the career academy program (1500 schools) and far larger than Upward Bound (727 schools) (Kemple and Snipes, 2000; Myers *et al*., 2004).

terms of attendance, grades or graduation rates. In El Paso schools participants had fewer disciplinary problems and better attendance, but lower SAT scores and college enrollments (Center for Strategic and International Studies, 1999).

A pilot program in 1992 sponsored by the US Departments of Education and Defense, which combined career academies with required JROTC participation, provides indirect evidence on JROTC (Hansen and Robyn, 2000). Elliott *et al*. (2002) find that students from these partnership academies have higher attendance, grades, and graduation rates. However, regular JROTC students (not also in a career academy) perform no different than general-track students not in a career academy or in a magnet school.

Pema and Mehay (2009a) is the only study to implement research strategies that address the self-selection of both students and schools into JROTC. They find negative program effects on high school graduation and postsecondary education alongside positive enlistment effects. However, they examine only broad program effects and assume equal program impacts on all participants, regardless of the extent or timing of their involvement in the program. This study examines whether program effects are heterogeneous depending on the intensity and the timing of participation.

THE DATA

For this study, we use data from the High School and Beyond (HSB) survey, which follows a representative sample of 14,825 sophomores from 1980 to 1992. Because HSB oversampled minorities and schools with high minority enrollments, this dataset offers larger sample sizes of JROTC participants than other nationally representative surveys. To our knowledge, HSB is the only survey that explicitly asks school administrators about the presence of JROTC in the school and students about their participation patterns.[4] In addition, HSB provides sample sizes that are sufficiently large to separate participants into sub-groups based on the intensity and timing of participation.

A key feature of JROTC is that enrollment is highest in the 9th grade but declines in each subsequent grade (Center for Strategic and International Studies, 1999). Only about one-quarter of all participants complete all four years of the program. Thus, it is likely that program effects will depend on when participation occurs and its duration. Most importantly, participation timing may be correlated with staying in school. For example, if participation is measured only during the last years of high school, it will be highly correlated (perhaps spuriously) with graduation.

The HSB survey provides snapshots of JROTC participation in both sophomore year and in senior year.[5] Based on this information, we construct multiple participation indicators. The first indicator equals one for students who were enrolled in JROTC any time during freshman or sophomore year. Some of these early participants stay in the program until graduation, whereas others leave. The second indicator equals one for those who were enrolled any time during junior or senior year. This indicator captures both students who stay in the program throughout high school as well as those who first join in 11th or 12th grade. We also create a third indicator to capture those who persist longer in the program based on those who respond

[4] Information on JROTC in other youth surveys, such as NELS or NLSY, can only be inferred from transcripts, which yields smaller samples of participants. In addition, other surveys do not collect information on whether a school offers JROTC.

[5] HSB asks students whether they have ever heard of or participated in a list of programs, one of which is JROTC. We define participants as those reporting to have participated, not simply heard of the program.

positively to the JROTC question in both the 1980 and 1982 surveys.[6] We assume that the majority of students who answer positively in both years participate in the program longer than those who respond positively to only one of the JROTC participation questions.

Our definition of participation indicators addresses two purposes. First, we account for program intensity and timing. Second, we obtain program effects that are comparable to those in the prior literature – both the limited literature on JROTC evaluation and the much larger program evaluation literature. The early literature on JROTC in particular, focuses on 'JROTC graduates,' those who complete high school as JROTC participants, regardless of whether they join the program late in high school or whether they participate throughout high school. Our results on 'late participants,' therefore, will be comparable to the early literature on JROTC evaluation (Center for Strategic and International Studies, 1999).

Analyzing graduation rates for late participants is problematic, since these students have survived until late (they were interviewed in the spring) in their final year. The only way to provide some insight into the school completion behavior of this group is to compare them with non-participants who survive to the same point. This strategy produces the appropriate control group, but does not allow us to draw any conclusions on the effect of the program on dropout behavior. This is because very few drop out in the final months of their senior year. Since dropout prevention is one of the program goals, we use the sophomore sample to investigate this outcome. School leaving laws generally prevent students from dropping out before 10th grade; therefore, comparing those who participate in JROTC in 9th or 10th grade to non-participants should provide insights into program effects on dropout rates. Finally, since the goal of evaluation studies normally is to examine the treatment effect for those who complete a program, we also analyze academic outcomes for continuous participants. For each participant definition we restrict the control group appropriately. When analyzing the sophomore sample, all respondents in the base survey year are included in the sample. When analyzing seniors we restrict the control group to include only students who remain in the same school through 12th grade. Thus, the senior sample excludes dropouts and early graduates.[7]

We also restrict the sample to students with non-missing information on JROTC participation.[8] Of the 332 JROTC enrollees in 1980 (2.5% of the sample) only 107 students persist in the program throughout high school. During 11th and 12th grade 126 new students enter the program. The summary statistics in Table I indicate that JROTC enrollees are more likely to be minority males attending urban public schools, who live with less-educated and lower-income parents, and who live in single-parent households.[9] These attributes highlight the at-risk status of JROTC students.

[6] Our continuous participation indicator is not as precise as we would like. Due to the wording of the question in HSB, it is possible that some who respond positively in either 1980 or 1982 remain in the program as long as those who respond positively in both years. However, we believe the number of such individuals is small because continuous participants appear very different in their observable characteristics from both early and late participants. In terms of estimation, the measurement error in the continuous participant indicator would, if anything, bias the program effect toward zero. In addition, one solution to measurement error is to instrument for the mismeasured variable, which is the approach we take in estimating causal effects.

[7] Since students most likely drop out due to poor academic outcomes, including them in the control group when treatment includes late JROTC participants would bias the results in favor of JROTC. A similar problem arises if we investigate program effects on dropout behavior by defining the early participant group to include only those who participate in 9th or 10th grade but no longer continue with the program. Removing continuous participants, who, by default, stay enrolled in high school, would negatively bias program estimates.

[8] About 1452 students (9.8% of the sample) have missing information on JROTC. They appear to be no different from non-participants in their observed characteristics.

[9] Table I presents unweighted summary statistics for the sample used in regressions. Regression results are also not weighted. Weighted summary statistics indicate that students who ever participate represent about 3.3% of the population of high school sophomores in 1980.

TABLE I Summary Statistics

	Sophomore Sample		*Senior Sample*							
	Early JROTC participants		*Non-JROTC sophomores*		*Late JROTC participants*		*Continuous JROTC participants*		*Non-JROTC seniors*	
Variable	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*
Female	0.37	0.48	0.52	0.50	0.39	0.49	0.43	0.50	0.51	0.50
Black	0.32	0.47	0.13	0.33	0.36	0.48	0.40	0.49	0.13	0.33
Hispanic	0.29	0.46	0.21	0.41	0.28	0.45	0.22	0.42	0.21	0.41
Parents completed high school	0.32	0.47	0.29	0.46	0.28	0.45	0.33	0.47	0.29	0.45
Parents have a college degree or higher	0.18	0.38	0.27	0.44	0.22	0.42	0.16	0.36	0.29	0.46
Single mother family	0.24	0.43	0.15	0.36	0.23	0.42	0.23	0.43	0.14	0.35
Bottom income category	0.14	0.34	0.07	0.25	0.10	0.30	0.09	0.29	0.05	0.22
Top income category	0.05	0.21	0.09	0.29	0.06	0.24	0.05	0.21	0.09	0.29
Attends urban school	0.49	0.50	0.23	0.42	0.41	0.49	0.56	0.50	0.22	0.42
Attends public school	0.95	0.22	0.78	0.42	0.88	0.32	0.98	0.14	0.74	0.44
Lives in the south	0.57	0.50	0.31	0.46	0.56	0.50	0.67	0.47	0.28	0.45
School hosts JROTC in 1980	0.71	0.46	0.14	0.35	0.63	0.48	0.93	0.25	0.13	0.34
School hosts JROTC in 1982	0.71	0.45	0.15	0.35	0.64	0.48	0.92	0.27	0.14	0.34
Test scores, 10^{th} grade	16.68	14.81	26.17	15.77	18.24	15.00	17.80	15.75	28.30	15.47
Test scores, 12^{th} grade	20.09	16.22	31.33	17.34	21.56	16.77	22.40	16.44	33.63	16.80
Disciplinary problems, 10^{th} grade	0.46	0.50	0.26	0.44	0.38	0.49	0.39	0.49	0.21	0.41
Disciplinary problems, 12^{th} grade	0.25	0.44	0.18	0.39	0.32	0.47	0.27	0.45	0.20	0.40
Drop out between 10^{th} and 12^{th} grade	0.33	0.47	0.17	0.37	0.00	0.00	0.00	0.00	0.00	0.00
High school diploma	0.63	0.48	0.82	0.39	0.89	0.31	0.90	0.29	0.96	0.19
Postsecondary enrollment	0.41	0.49	0.63	0.48	0.49	0.50	0.47	0.50	0.71	0.46
Enlist in the military	0.21	0.41	0.08	0.27	0.23	0.42	0.26	0.44	0.08	0.26
Sample size	332		12,901		233		107		11,110	

We use the base year (1980) to observe the characteristics of students, their families, and the schools they attend.[10] We chose the 1984 follow-up two years after high school to evaluate post-secondary outcomes, assuming that the program would have the greatest impact on education and work choices made immediately after high school.

We select outcomes that mirror the stated goals of JROTC (Bailey *et al*., 1992; Center for Strategic and International Studies, 1999; Denver Public Schools, 1996; Glover, 2002). The outcomes include disciplinary problems, dropout rates, graduation probabilities, postsecondary enrollments, and enlistments. We also analyze test scores in 12th grade and gains in test scores between 10th and 12th grade. Test scores are based on standardized tests administered in 1980 and 1982 to all respondents (including dropouts) in reading, writing, vocabulary, mathematics, science, and civics.[11] Test scores in 12th grade represent the cumulative effect of individual ability, motivation, school resources, as well as JROTC participation. However, test score gains between 10th and 12th grade net out individual and school inputs, thus resulting in potentially stronger estimates of the JROTC effect. This is especially important if we believe that JROTC students are negatively selected based on individual characteristics, and if they attend schools with fewer resources.

We measure postsecondary enrollments in 1984, two years after high school. Postsecondary institutions include 2- and 4-year colleges and vocational schools. Enlistments include all students reporting as serving on active duty or reserves at any time after high school in any follow-up from 1982 to 1992.[12]

Two alternative graduation measures are constructed, based on the treatment of dropouts. The first indicator uses the 'event' definition of graduation and identifies dropouts as those who leave school even if they later return (National Center for Education Statistics, 1987). The 'status' definition evaluates a student's status at a given point in time and counts students who have earned a diploma two years after high school as graduates even if they previously had dropped out. In the status definition we treat GED recipients as non-graduates.

Table I indicates that JROTC members generally have higher dropout rates and lower high school completion rates than non-participants. Postsecondary enrollments are lower for JROTC students, but military enlistments are much higher.

Appendix Table A1 focuses on only JROTC schools and compares participants with non-participants within those schools. While JROTC schools are mostly urban, located in the south, and appear to have more disadvantaged students, JROTC students appear to be negatively selected even within these schools in terms of observable characteristics. These differences in outcomes, however, are merely descriptive and do not represent program effects. Below we explore potential biases from the non-random selection of schools and students into the program.

ESTIMATION AND SELECTION ISSUES

Estimating the effect of JROTC on student outcomes is complicated by the potential endogeneity of participation. JROTC enrollees may differ systematically in their academic backgrounds, attitudes, or interests. For example, if JROTC is attended predominantly by

[10] Where possible, we fill in missing information in the base year with answers to the same questions collected in later follow-ups.

[11] We focus on the sum of the 'formula' score on the vocabulary, reading, and the first part of the mathematics test.

[12] To be consistent with the measurement of the rest of the outcomes we also obtained results for enlistment rates two years after high school. The results were very similar to those that used the later follow-ups, suggesting that most enlistments occur soon after high school.

under-performing students, estimates may be negatively biased. To disentangle the effect of JROTC on academic performance from unobserved factors that may drive both enrollment decisions and academic outcomes, we jointly model enrollment decisions and academic performance. For student i attending school j we specify the following model:

$$Y_{ij} = 1(\alpha JROTC_{ij} + \beta_1 X_i + \beta_2 S_j + \beta_3 FEDPROG_{jt} + u_{ij} > 0) \quad (1)$$

$$JROTC_{ij} = 1(\pi_1 X_i + \pi_2 S_j + \pi_3 SJROTC_{jt} + \pi_4 FEDPROG_{jt} + \nu_{ij} > 0) \quad (2)$$

$$\begin{bmatrix} u_{ij} \\ \nu_{ij} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right) \quad (3)$$

Y_{ij} includes academic outcomes, X_i includes demographics (age, race, gender), family background (parents' income, education, and family structure), and S_j includes school attributes (urban location, ownership, region, percentage enrollment of disadvantaged students, percentage faculty with advanced degrees). In addition, we include a variable ($FEDPROG_{jt}$) for the number of other federally-funded programs available at the school in time t, 1980 or 1982 (discussed below). Equation (2) assumes that enrollment decisions depend on individual characteristics (X_i), school characteristics (S_j), whether the school offers JROTC in 1980 or 1982 ($SJROTC_{jt}$), and a proxy for other federal programs at the school ($FEDPROG_{jt}$). Identification requires instrumental variables (IVs) that predict participation in JROTC, but that are uncorrelated with unobserved factors associated with educational outcomes.

Indicators for whether the school hosts JROTC in 1980 or in 1982 will be valid instruments under certain assumptions. First, school offerings of JROTC should influence individual participation. In estimates presented below, this condition appears to hold as the first stage regression of $JROTC_{ij}$ on $SJROTC_{jt}$ always finds statistically significant coefficients for the latter and a strong partial correlation between individual participation and school offerings. The second condition for a valid instrument is that $E[Y_{ij} \mid JROTC_{ij}, X_i, S_j, FEDPROG_{jt}]$ should not depend on whether the school offers JROTC. Ideally, if schools randomly offered the program, $SJROTC_{jt}$ would not affect individuals' academic outcomes other than via individual enrollment. Therefore, in the absence of school-level selection into JROTC, this model would adequately address student self-selection. However, if JROTC programs are not randomly distributed across schools, the concern is that, since JROTC is oriented toward disadvantaged youth, the program's presence will be correlated with other at-risk characteristics of the student body.

To deal with this concern, in equation (1) we explicitly control for the number of other federal programs the school offers ($FEDPROG_{jt}$). These initiatives include Talent Search, Upward Bound, the Vocational Education Act of 1963, the Elementary and Secondary Education Act (ESEA) (of which we include Titles I (Improving the Academic Achievement of the Disadvantaged), Title IV B-D (Library, Educational Innovation, Supplementary Centers), Title VII (Bilingual Education), and Title IX (Ethnic Heritage Studies)). Because these programs target similar at-risk populations and focus on improving academic outcomes, their inclusion in equation (1) allows the effect of our IV to emerge only via individual participation in JROTC. The number of federal programs offered proxies for student demographics (other

than the ones we control for explicitly) that affect the school's ability to qualify for these programs and that are also correlated with academic achievement.[13]

Data from HSB school surveys supports our claim that, conditional on the number of federal initiatives and other school controls, school participation in JROTC can be treated as random. Of 1032 schools, about 98% participate in at least one of the federally-funded programs.[14] All schools that host JROTC also participate in one of the federal initiatives. In addition, JROTC schools on average participate in one less federal program than non-JROTC schools,[15] suggesting some substitutability between JROTC and other federally-funded programs. Schools hosting JROTC appear to have similar student demographics to schools that host an average number of other federal programs. The difference in these programs is the career path that they aim to influence – academic achievement, employment, or enlistments.

The enlistment model poses a different problem because schools with JROTC may have more pro-military attitudes. If so, JROTC could affect enlistments through channels other than via individual enrollment and stimulate spillover effects on overall enlistment rates. Therefore, for the enlistment model we alter the specification of equation (1):

$$Enlist_{ij} = 1(\alpha JROTC_{ij} + \beta_1 X_i + \beta_2 S_j + \beta_3 SJROTC_{jt} + u_{ij} > 0) \tag{1a}$$

To deal with the possibility that individual enlistment may be due to aggregate school (or peer) effects, we explicitly control for the presence of JROTC at the school in equation (1a). The omission of $FEDPROG_{jt}$ in equation (1a) represents the exclusion restriction for this model. We assume that, after conditioning on $SJROTC_{jt}$, $FEDPROG_{jt}$ is not systematically correlated with school-level characteristics that influence enlistments. However, $FEDPROG_{jt}$ and $JROTC_{ij}$ should be correlated because students will be less likely to join JROTC in schools that offer more alternative programs (and, therefore, more choice). Since we control for the socioeconomic status of both the individual and the school's student body, we control for the possibility that schools offering such initiatives have more disadvantaged students.

Our identification strategy assumes that each federal program will have the largest effect on its targeted outcome – academic-oriented programs on academic achievement, vocational programs on employment, and JROTC on enlistments. These programs may affect student outcomes through both individual participation and also through peer or school-wide (aggregate) effects. However, conditional on the existence of other (academic) federal programs, JROTC should have no indirect (school-wide) effect on academic outcomes beyond its effect via individual participation. Similarly, academic programs should have no indirect effects on enlistments after controlling for the presence of JROTC in the school.[16]

We include two different indicators of school participation in JROTC and other federal programs (in 1980 and 1982) instead of a single indicator (such as 'ever participate') for two reasons. First, this specification can explain why some students enroll in JROTC only in

[13] Talent Search targets disadvantaged students and aims to reduce dropout rates and increase postsecondary education. Upward Bound serves students from low-income families, students whose parents have less than a bachelor's degree, and low-income, first-generation military veterans who are preparing to enter postsecondary education. Upward Bound aims at boosting high school graduation and postsecondary enrollments. Some funds were also earmarked for occupational training to potential high school dropouts and for developing work-study programs. ESEA is a precursor to No Child Left Behind and Title I targeted schools with disadvantaged students.

[14] The average school offers seven federal programs. The 18 schools that do not offer any federal programs are very small (maximum enrollment 282). If we include JROTC as one of these programs, only 1% of the school sample does not participate in one of these initiatives at some point in time. JROTC is offered by 191 schools (18% of the sample), of which 23 add or drop the program during 1980–1982.

[15] This result is obtained from a regression of the number of federal programs on 'school offers JROTC,' which also includes controls for school size, urban location, private ownership, and region (results available on request).

[16] Note that we are not assuming away the existence of aggregate effects of these programs, only that aggregate effects are confined to the specific outcomes targeted by each program.

certain grades, thus aiding the estimation of treatment effects based on the timing of participation. Second, including both indicators as IVs allows us to perform overidentification tests on the exogeneity of the instruments.[17]

Instrumentation recovers average treatment effects on academic outcomes from students who join JROTC because the school offers the program. Similarly, we obtain enlistment effects from students who join JROTC because few alternative programs are offered. Since these groups represent subpopulations of all treated, these local average treatment effects (LATE) may differ for other students (those who would never enroll in JROTC and those who would seek out JROTC regardless of whether the high school offers it). However, it is informative for an overall evaluation of JROTC to obtain treatment effects for different subgroups.

Students can participate in JROTC in any grade and for any length of time (from one to eight semesters). Since the motivation and self-selection of students may change over time, defining participation as 'ever in JROTC' will not capture effects for subgroups of enrollees with widely varying program exposure. We use the variables for the three sub-groups of participants (early, late, and continuous) to determine whether the timing and intensity of enrollment produces different program effects. In separating the sample this way, we must account for the fact that JROTC participation is right-censored for dropouts. Since HSB follows dropouts, transfers, and early graduates we can observe all outcomes for school leavers. However, JROTC enrollment is only observed for students who remain in school. Therefore, estimates of the JROTC effect on school completion for late participants may be biased upward if the control group includes dropouts. To deal with this problem, for late participants we restrict the control group to non-participants who also survive until senior year.

PRELIMINARY MULTIVARIATE ANALYSES

We first present preliminary estimates from single-stage models that include an extensive set of proxies to control for unobserved characteristics affecting participation and outcomes. Wooldridge (2002) describes the conditions under which the use of proxy variables for unobservables can lead to consistent estimates of the parameters of interest. We present these results as a starting point for assessing the size and direction of JROTC effects, even though, for reasons discussed above, it is unlikely that this approach eliminates all bias from unobservables.

Table II summarizes preliminary OLS and probit estimates for each participant group. For continuous participants the correct comparison group is not clear-cut. One approach would compare their outcomes to non-participant seniors who also survive to 12th grade. This eliminates the risk of confounding the program effect with survival in high school. Alternatively, the fact that continuous participants remain in school may represent a program effect, which argues for comparing completers to the original (sophomore) sample. Since both approaches have merit, we compare continuous participants to both sophomores and to the (restricted) sample of seniors.

OLS results indicate that all JROTC participants have more disciplinary problems. They also have lower test scores in 12th grade. However, differences in test score gains are insignificant except for late participants, whose 12th grade scores drop by one point relative to 10th grade scores. These results indicate that test score levels may include a larger downward bias than test score gains. The preliminary results also suggest that early participants are more

[17] Including a single dummy for whether the school ever had a JROTC program made no difference in the estimated program effects.

TABLE II Preliminary OLS/Probit Estimates

Treatment Group:	*Early Participants*	*Late Participants*	*Continuous Participants*	
Control Group:	*Sophomores*	*Seniors*	*Sophomores*	*Seniors*
OLS estimates				
Test scores in 12th grade	–3.872	–7.168	–2.344	–4.890
	(0.953)***	(1.108)***	(1.326)*	(1.350)***
Test score gains	–0.101	–1.459	–0.389	–0.195
(12th grade score – 10th grade score)	(0.584)	(0.646)**	(0.911)	(0.896)
Probit estimates				
Any disciplinary problems in 12th grade	0.186	0.321	0.339	0.246
	(0.094)**	(0.095)***	(0.134)**	(0.142)*
	[0.052]	[0.098]	[0.100]	[0.073]
Any disciplinary problems in 12th grade	0.082	0.250	0.378	0.193
(controlling for 10th grade problems)	(0.106)	(0.102)**	(0.134)***	(0.140)
	[0.021]	[0.071]	[0.109]	[0.053]
Drop out of high school	0.161	n.a.	n.a.	n.a.
	(0.090)*			
	[0.034]			
High school diploma	–0.225	–0.316	0.933	–0.085
	(0.104)**	(0.138)**	(0.245)***	(0.204)
	[–0.049]	[–0.023]	[0.098]	[–0.005]
Postsecondary enrollment	–0.188	–0.319	–0.176	–0.358
	(0.092)**	(0.114)***	(0.161)	(0.158)**
	[–0.069]	[–0.107]	[–0.064]	[–0.121]
Enlist in the military	0.472	0.468	0.693	0.619
	(0.097)***	(0.119)***	(0.157)***	(0.169)***
	[0.071]	[0.070]	[0.120]	[0.101]

Notes: All regressions include demographics (age, gender, race), family (parents' education, parents' income, family composition), school characteristics (urban location, private ownership, percent students disadvantaged, percent faculty with graduate degrees), and region. Regressions for high school diploma, postsecondary enrollment, and enlistments include test scores in 10th grade as a proxy for ability. Regressions also include separate categories for missing controls to avoid dropping observations with missing information. Standard errors appear in parentheses and are robust to within-school serial correlation. Binary response models are estimated via probit and partial effects appear in square brackets.
***significant at 1%; **significant at 5%; *significant at 10%.

likely to drop out between 10th and 12th grade, and that both early and late participants are less likely to obtain a high school diploma two years after high school.

When compared with non-participant sophomores, continuous participants are more likely to earn a diploma. The concern is that this effect is spurious, which is supported by the absence of differences in graduation rates when continuous participants are compared to seniors. In line with these findings, JROTC participants are less likely to pursue postsecondary education, with the exception of continuous participants, whereas all JROTC sub-groups are more likely to enlist.

These preliminary results are most likely negatively biased because JROTC participants are negatively selected on observables, and most likely are negatively selected on unobservables as well. Similarly, the enlistment effect may be overestimated if the program simply identifies students with stronger military preferences who would have enlisted anyway. However, enlistment effects could also be underestimated if we account for the demand side of recruiting, since JROTC students with poor academic achievement are not likely to qualify for admission to the military. Due to these potential biases, the preliminary estimates should not be used to infer causal program effects or to guide policy. The next section adjusts for such biases.

SELECTION-ADJUSTED ESTIMATES

Panel A of Table III presents estimates of test score gains obtained via two stage least squares (2SLS), while Panel B presents results of bivariate probit models for the binary outcomes. All models indicate higher test scores for JROTC participants and positive test score gains between 10th and 12th grade. In particular, early and late participants realize test score gains of 7 points (one-half a standard deviation), while continuous participants achieve gains of 11 to 13 points, depending on the comparison group. In our second stage regressions of 12th grade test scores on all controls, $FEDPROG_{jt}$ has a positive coefficient, consistent with the argument that academic programs have school-wide effects on student performance. However, when focusing on test score gains, $FEDPROG_{jt}$ becomes insignificant in most specifications, suggesting that test score gains net out both individual ability and (observable and unobservable) school inputs.

Panel A of Table III reports the F-statistics on the joint significance of our instruments and indicates they are highly significant. In addition, the partial R^2 from the first stage regression that measures the correlation between the instruments and the endogenous variable suggests that the instruments jointly explain 3.5 to 4% of the variation in participation. This indicates that both instruments are strong predictors of individual program participation. We also perform endogeneity tests for JROTC participation in model (1) and overidentification tests for the IVs. The endogeneity test examines the correlation in the disturbances of equations (1) and (2) in the 2SLS models of 12th grade test scores and test score gains; in all cases the correlation is significant, suggesting that $JROTC_{ij}$ is endogenous in equation (1) and that instrumentation is necessary for the test score models. Overidentification tests support the validity of our assumption of exogeneity of the instrument ($SJROTC_{jt}$) in the academic achievement models.

Inclusion of the two instruments for a school hosting JROTC (in 1980 and 1982) sheds some light on differences between participant subgroups and also helps identify the source of the treatment effects. In equation (2), $SJROTC_{j,1980}$ is significant for early participants, but $SJROTC_{j,1982}$ is insignificant. These results are tabulated in Appendix Table A2, which also presents detailed results for one sub-group (early enrollees) and one outcome (graduation).[18] For late participants the reverse is true – $SJROTC_{j,1980}$ is insignificant, but $SJROTC_{j,1982}$ is

[18] The full range of results for equation (2) for all sub-groups and outcomes are available on request.

TABLE III Selection Adjusted (2SLS and Bivariate Probit) Estimates

Treatment group:	*Early participants*		*Late Participants*		*Continuous Participants*			
Control group:	*Sophomores*		*Seniors*		*Sophomores*		*Seniors*	
Panel A. 2SLS estimates	$\hat{\alpha}$	*F-stat*	$\hat{\alpha}$	*F-stat*	$\hat{\alpha}$	*F-stat*	$\hat{\alpha}$	*F-stat*
Test scores in 12th grade	10.262	202.52	9.848	129.79	23.767	159.80	13.384	161.58
	(4.868)**		(6.281)		(8.427)***		(7.762)*	
Test score gains	7.342	156.63	7.183	114.08	12.608	136.06	10.567	139.32
(12th grade score – 10th grade score)	(3.286)**		(3.884)*		(5.305)**		(4.895)**	
Panel B. Bivariate Probit estimates	$\hat{\alpha}$	$\hat{\rho}$	$\hat{\alpha}$	$\hat{\rho}$	$\hat{\alpha}$	$\hat{\rho}$	$\hat{\alpha}$	$\hat{\rho}$
Disciplinary problems, 12th grade	0.052	0.033	0.298	0.012	–0.087	0.200	–0.173	0.209
	(0.331)	(0.159)	(0.399)	(0.188)	(0.422)	(0.208)	(0.402)	(0.208)
	[0.014]		[0.091]		[–0.021]		[–0.043]	
Disciplinary problems, 12th grade	0.025	0.003	0.376	–0.051	0.197	0.079	–0.136	0.169
(controlling for 10th grade problems)	(0.319)	(0.150)	(0.370)	(0.171)	(0.476)	(0.228)	(0.404)	(0.205)
	[0.006]		[0.112]		[0.053]		[0.032]	
Drop out of high school	–0.420	0.292	n.a.		n.a.		n.a.	
	(0.220)*	(0.108)						
	[–0.063]							
High school diploma	0.432	–0.322	0.397	–0.382	1.572	–0.330	0.514	–0.351
	(0.246)*	(0.120)	(0.370)	(0.196)	(0.408)***	(0.210)	(0.411)	(0.228)
	[0.063]		[0.014]		[0.110]		[0.016]	
Postsecondary enrollment	–0.371	–0.089	–0.390	0.019	–0.447	0.127	–0.537	0.085
	(0.280)	(0.133)	(0.340)	(0.159)	(0.404)	(0.192)	(0.403)	(0.206)
	[–0.140]		[–0.134]		[–0.170]		[–0.189]	
Enlist in the military	0.943	–0.192	0.343	0.084	1.125	–0.181	0.678	–0.001
	(0.377)**	(0.172)	(0.547)	(0.257)	(0.516)**	(0.235)	(0.543)	(0.268)
	[0.194]		[0.048]		[0.25]		[0.118]	

Notes: All models include demographics, family, and school characteristics. Academic outcome models also include the number of federal initiatives as an additional regressor. Test scores are estimated via two-stage least squares, whereas the other outcomes are estimated via bivariate probit. The instruments used for academic outcomes include two separate dummies indicating whether the school hosts JROTC in 1980 and in 1982. For the enlistment models, the instruments include the number of federal initiatives offered in 1980 and in 1982. In addition, enlistment models explicitly control for whether a school has a JROTC unit. Standard errors appear in parentheses, and marginal effects appear in square brackets.
***significant at 1%; **significant at 5%; *significant at 10%.

significant. For continuous JROTC participants both $SJROTC_{j,1980}$ and $SJROTC_{j,1982}$ are significant and positive. Therefore, it appears that intermittent enrollees are not affected by whether the school hosts the program in the years when they do not participate, suggesting that other reasons affect their enrollment decision. Continuous participants, on the other hand, are those who are most affected by program availability at the school.

Panel B of Table III presents estimated program effects ($\hat{\alpha}$) and the estimated correlation in the disturbances from the two equations ($\hat{\rho}$). These estimates reveal no significant differences in disciplinary problems across any of the subgroups. However, there is evidence that the program improves school completion. Early participants are less likely to drop out (by 6.3 points), and are more likely to obtain a diploma. Continuous participants are 11 points more likely to graduate if we treat their persistence in school as a program effect, and compare them to other sophomores; however, there is no difference when continuous participants are compared to non-participant seniors. We detect no significant graduation effects for late participants.[19] Similarly, we observe no differences in postsecondary enrollment for any subgroup.

Because $SJROTC_{jt}$ may be correlated with pro-military attitudes at the school, we explicitly control for school participation in JROTC in the enlistment models and instrument individual enrollment with $FEDPROG_{jt}$ (for 1980 and 1982). These variables proxy for the menu of alternative programs aimed at disadvantaged students and, therefore, should be correlated with individual JROTC enrollment. Estimates of equation (2) indicate that JROTC and other federal programs are substitutes. In Table A2, early JROTC participants are *less* likely to participate in other federal programs in 1980 (while they are participating in JROTC), but are *more* likely to participate in such programs in 1982 (when they drop JROTC).

The enlistment models find that early and continuous participants are 19.4 and 25.0 points, respectively, more likely to enlist than non-participant sophomores (compared with the 7–11 point differences in OLS). This evidence supports the view that unobserved at-risk characteristics of JROTC participants render them less likely to be recruited, and that instrumentation addresses this problem. Indeed, the coefficient of 10th grade test scores in the enlistment model is positive and significant, suggesting that those with better academic performance are more likely to qualify for enlistments.

It is noteworthy to examine the coefficients of $SJROTC_{j,1980}$ and $SJROTC_{j,1982}$ for their effects on individual enlistment probabilities, since this sheds light on any program spillover effects. In all regressions, $SJROTC_{j,1980}$ has a negative impact on enlistments, whereas $SJROTC_{j,1982}$ has a positive impact.[20] Enlistments appear to increase when schools add JROTC in the year when the student is a senior. However, if the school offers the program in 1980 but drops it in 1982, the predicted effect on enlistments is negative. This result is not surprising, since the decision to drop the program may be associated with unfavorable military attitudes in the district. If the school offers the program in both years, the spillover effect is positive but small (0.2 points). Overall, this evidence suggests that the main enlistment effect of the program comes from individual participation, rather than via school-level spillovers.

[19] To assess whether graduation effects derived from the IV approach are sensible, we follow Altonji *et al*. (2005) and construct bounds around the estimates. This approach assumes that any unobserved heterogeneity is, at most, of the same magnitude as observed heterogeneity. Since JROTC students are negatively selected, unobserved characteristics would bias graduation effects downward. Therefore, OLS estimates would provide a lower bound, and estimates obtained under the Altonji *et al*. assumption would provide an upper bound. After implementing this approach, we find that graduation effects for early participants could be as large as 10 points higher, which suggests that our selection-adjusted estimates are in the right direction and, if anything, may underestimate the true program effect.

[20] Both are significant. In a probit enlistment model for early participants $SJROTC_{j,1980}$ has a partial effect of –0.028 (0.11), whereas $SJROTC_{j,1982}$ has a partial effect of 0.03 (0.15). Results are available upon request.

The correlation of disturbances across the equations is negative when analyzing graduation and other postsecondary outcomes, but positive when analyzing disciplinary problems and dropout behavior. This is consistent with the view that JROTC enrollees are more at-risk than non-participants. However, the disturbances in the two equations are significantly correlated only in the case of dropout behavior and acquisition of a diploma, suggesting that the baseline (uninstrumented) estimates for the remaining binary outcomes may be reliable.

SCHOOL-LEVEL ESTIMATES

The previous section suggested that school offerings of JROTC may increase enlistments for non-participants as well as participants. The recruiting effect of the program is its main source of controversy, with opponents claiming that the presence of JROTC in schools improves information on military careers and, therefore, boosts enlistments. However, this may not be a causal program effect. If JROTC is found in schools with more positive attitudes toward the military, then higher enlistments in such schools may not be entirely due to the program's presence. To obtain a causal effect of $SJROTC_j$ on aggregate enlistments, we propose the following model:

$$\%\ Students\ Enlist_j = \gamma\ SJROTC_j + \delta S_j + \eta_j \tag{4}$$

$$SJROTC_j = \phi_1 S_j + \phi_2 FEDPROG_j + \zeta_j \tag{5}$$

In equation (4) we measure program participation with a single variable indicating whether the school hosted a JROTC unit at any point in time during the observation period.[21] In equation (4) we also control for various school characteristics.[22] Including the school controls ensures that $SJROTC_j$ does not capture the effect of school quality or the student body composition.

We instrument the presence of JROTC in the school with the number of other federal programs (FEDPROG). From the discussion earlier, JROTC and other federal programs appear to be correlated, since schools that adopt numerous federal initiatives may be less likely to apply for JROTC. Alternatively, if a school has a large at-risk population it may be more likely to offer both several federal initiatives and JROTC. For the purposes of identification, it does not matter whether the correlation between $SJROTC_j$ and $FEDPROG_j$ is positive or negative, as long as it is significant. The exclusion restriction requires that other federal programs have no impact on school enlistments other than by identifying schools likely to host JROTC.

Table IV presents OLS (column 1) and 2SLS estimates (column 2) of school-wide enlistments. Column 3 summarizes the main variables from the school sample. Both OLS and 2SLS results indicate that schools hosting JROTC have higher aggregate enlistment rates. The magnitude of the difference varies between 0.8 and 5.63 percentage points, which is sizeable, given that the average school-wide enlistment rate is 4%. The first stage regression indicates that the instrument significantly predicts school participation in JROTC. The partial R^2 from the first stage regression indicates that other federal initiatives explain about 2% of the

[21] The variation obtained from schools adding and dropping the program over time is too small when working with the school survey.

[22] To reduce measurement error we average over time the percentages of students who enlist, the percentage disadvantaged, and the percentage of faculty with advanced degrees.

TABLE IV School-level Enlistments

Dependent variable:	*OLS estimates*	*2SLS estimates*	*Sample average (std. dev.)*
Percent of graduating class that enlists			3.96
			(3.22)
School offers JROTC	0.824	5.632	0.19
	(0.272)***	(2.569)**	(0.39)
Public	1.967	1.082	0.87
	(0.304)***	(0.587)*	(0.34)
Urban	0.436	–0.388	0.25
	(0.252)*	(0.526)	(0.43)
Rural	0.974	1.495	0.27
	(0.237)***	(0.390)***	(0.44)
General academic program	0.908	0.909	0.92
	(0.443)**	(0.514)*	(0.26)
Vocational program	2.653	3.471	0.02
	(0.786)***	(1.010)***	(0.14)
Percent students disadvantaged	0.028	0.022	18.02
	(0.005)***	(0.006)***	(22.48)
Percent faculty with a graduate degree	–0.013	–0.016	48.30
	(0.004)***	(0.005)***	(23.93)
Region dummies	yes	yes	
Observations	871	871	1,032
F-statistic on: H_0: *instruments are jointly zero*	—	13.13	
Instrument:			
Number of federal programs in school during 1980–1982			6.84
			(3.27)

Notes: Estimates were obtained using the school sample. All variables were averaged over the 1980 and 1982 period to reduce measurement error. The indicator 'school offers JROTC' measures the presence of a JROTC unit in either 1980 or 1982.
***significant at 1%; **significant at 5%; *significant at 10%.

probability that a school hosts JROTC. The endogeneity test also provides evidence that $SJROTC_j$ is endogenous in equation (4), thus we favor the 2SLS estimates.

CONCLUSIONS

This study employs nationally representative data to estimate the effects of high school JROTC on student achievement and military enlistment. Relevant to program evaluation research is the finding that program effects vary between short- and long-duration participants and between those who participate in the early grades versus those who participate in later grades. We find that all participants improve their test scores. However, only those who enroll early in the program also improve their graduation rates. Early and continuous participants are far more likely to enlist than their peers, but we find no enlistment differences for late participants.

Our findings of higher enlistments and lower postsecondary enrollment for JROTC students are similar to those of Pema and Mehay (2009a). However, unlike the earlier study, we find positive academic outcomes for two JROTC sub-groups – early and continuous participants. In part this is likely due to our ability to account for the intensity and timing of program involvement, which may identify effects that are obscured when a single participation

indicator (such as 'ever participate') is used. Another explanation for the difference between the two studies is that our IV approach recovers treatment effects from students who participate due to the presence of JROTC at the school (marginal participants), rather from than the average JROTC student.

We also estimate program effects at the school level and find that overall enlistment rates of the graduating class are higher in schools hosting JROTC. This effect is quite large if we instrument for selection at the school level. It should be noted though, that, while marginal enlistment effects are large, they are practically small in absolute value – less than a third of the students with some exposure to JROTC enlist, and only about 10% of the graduating class in JROTC schools enlist.

Does the JROTC program pay off for the military or for society? From the recruitment standpoint, the annual cost to DOD of the JROTC program is only about $454 per cadet. Of course, the cost of a student who completes all four years is much higher because only a fraction of the annual population of enrollees graduates each year. Nonetheless, our estimated marginal effects, even after controlling for selection, indicate that JROTC continuous participants are far more likely to enlist. Moreover, recent data indicates that the direct cost of recruiting a 'high quality' accession (those with a traditional high school diploma and AFQT test scores above the 50th percentile), which represents the military's target market, can be as high as $19,275 (Moore *et al*., 2001). Under a set of reasonable assumptions, our estimates suggest that JROTC may be a cost-effective alternative to regular recruitment programs.[23]

In addition, other direct benefits may accrue to the military from the military-specific training received in JROTC. New recruits with JROTC backgrounds may be more successful and more productive than other recruits. To analyze this effect, we used Defense Manpower Data Center data on recruiting cohorts (Navy only) for the period 1990–2000. We tracked all entrants until separation or the end of their enlistment contract. Our results indicate that new recruits with JROTC backgrounds have higher first term completion rates, and that those who complete their contracts are more like to re-enlist (Pema and Mehay, 2009b). Lower first-term attrition and increased reenlistments will translate into lower replacement costs to DOD.

To gauge social efficiency, the cost-benefit assessment will need to incorporate the portion of program costs covered by local school districts, which nearly doubles the cost per cadet, and also must attempt to capture the full range of social benefits. While it is difficult to place a dollar value on all of the benefits generated by this multidimensional program, our estimates indicate that JROTC improves the acquisition of a high school diploma (and reduces dropout rates) for early participants. This represents an important program outcome, as reducing dropout rates has been an elusive outcome for most prior federal dropout prevention programs (Dynarski, 2004). Moreover, improving high school completion can have substantial long-run economic benefits in terms of higher lifetime earnings (Lavy and Schlosser, 2005).

ACKNOWLEDGEMENTS

The authors thank David Neumark, Barry Hirsch, Alice Crawford, Linda Bailey, Dahlia Remler, Yu-Chu Shen, and seminar participants at Baruch College/CUNY, and at the 2008 WEAI Conference for helpful comments. Yee-Ling Ang and Janet Days provided research assistance.

[23] If we assume an enrollment of 500,000 students each year, that roughly 13% graduate as JROTC participants, and that among these 30% enlist (a lower bound estimate, see Center for Strategic and International Studies, 1999), the cost to DOD per new recruit via JROTC would be about $11,350, which compares favorably with the average cost via direct recruitment programs.

References

Altonji, J.G., Elder, T.E. and Taber, C.R. (2005) Selection on observed and unobserved variables: assessing the effectiveness of catholic schools. *Journal of Political Economy* **113**(1) 151–184.

Atkins, K. (2005) Role models or recruiters? Expanding JROTC still has critics. *Boston Herald,* October 18, p. 8.

Bailey, S., Hodak, G., Sheppard, D. and Hassen, J. (1992) *Benefits Analysis of NJROTC* (Technical Report 92-015). Orlando, FL: Naval Training Systems Center.

Barron, J.M., Ewing, B.T. and Waddell, G.R. (2000) The effects of high school athletic participation on education and labor market outcomes. *Review of Economics and Statistics* **82**(3) 409–421.

Berlowitz, M.J. and Long, N.A. (2003) The proliferation of JROTC: educational reform or militarization? In *Education as Enforcement: The Militarization and Corporatization of Schools,* edited by K. Saltman and D. Gabbard. New York: Routledge.

Blundell, R., Dearden, L. and Sianesi, B. (2004) *Evaluating the Impact of Education on Earnings in the UK: Models, Methods, and Results from the NCDS*. Institute for Fiscal Studies, University College London.

Center for Strategic and International Studies (1999) *Junior Reserve Officers Training Corps*. Washington, DC.

Coumbe, A. and Harford L. (1996) *The U.S. Army Cadet Command: The 10-Year History*. Ft. Monroe, VA: US Army Cadet Command.

Crawford, A., Thomas, G. and Estrada, A. (2004) *Best Practices at Junior Reserve Officers Training Corps Units*. Monterey, CA: Naval Postgraduate School, Graduate School of Business and Public Policy.

Denver Public Schools (1996) *Junior Reserve Officers Training Corps (JROTC) Program Evaluation*. Denver, CO.

Dynarski, M. (2004) Interpreting the evidence from recent federal evaluations of dropout prevention programs: the state of scientific evidence. In *Dropouts in America,* edited by G. Oldfield. Cambridge, MA: Harvard Press.

Elliott, M.N., Hansen, L.M., and Gilroy, C.L. (2002) *Evidence of Positive Student Outcomes in JROTC-Career Academies*. Santa Monica, CA: Rand Corporation.

Glover, C. (2002) Army JROTC. Unpublished report. Ft. Monroe, VA: U.S. Army Cadet Command.

Hansen, L.M. and Robyn, A. (2000) *Implementing High School JROTC Career Academies*. Santa Monica, CA: Rand Corporation.

Kemple, J.J. and Snipes, J.C. (2000) *Career Academies: Impacts on Students' Engagement and Performance in High School*. New York: Manpower Demonstration Research Corporation.

Lavy, V. and Schlosser, S. (2005) Targeted remedial education of underperforming teenagers: costs and benefits. *Journal of Labor Economics* **23** 839–874.

Moore, C., Golding, H. and Griffis, H. (2001) *Manpower and Personnel IWAR 2000: Aging the Force*. Alexandria, VA: Center for Naval Analyses.

Myers, D., Olsen, R., Seftor, N., Young, J. and Tuttle, C. (2004) *The Impacts of Regular Upward Bound*. Washington, DC: Mathematica Policy Research.

National Center for Education Statistics (1987) *Who Drops Out of High School? Findings from the High School and Beyond*. Washington, DC: U.S. Department of Education.

National Center for Education Statistics (2006) *The Adult Lives of At-Risk Students*. Washington, DC: U.S. Department of Education.

Nazario, S. (2007) Junior ROTC takes a hit in L.A. *Los Angeles Times,* February 19.

Pema, E. and Mehay, S. (2009a) The effect of high school JROTC on student achievement, educational attainment, and enlistment. *Southern Economic Journal* October forthcoming.

Pema, E. and Mehay, S. (2009b) What does occupation-related vocational education do? Evidence from an internal labor market. Working paper. Monterey, CA: Graduate School of Business and Public Policy, Naval Postgraduate School.

Tucker, J. (2006). School board votes to dump JROTC program. *San Francisco Chronicle,* November 15, p. 1.

US Department of Defense (2008) *Department of Defense Budget FY2009 — Military Personnel Programs and Operation and Maintenance Programs*. Washington, DC.

Wooldridge, J. (2002) *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

APPENDIX

TABLE A1 Differences between Participants and Non-participants in JROTC Schools

	Sophomores in JROTC schools				*Seniors in JROTC schools*					
	Early JROTC participants		*Non-JROTC sophomores*		*Late JROTC participants*		*Continuous JROTC participants*		*Non-JROTC seniors*	
Variable	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*	*Mean*	*Std. dev.*
Female	0.38	0.49	0.53	0.50	0.41	0.49	0.44	0.50	0.52	0.50
Black	0.36	0.48	0.19	0.40	0.46	0.50	0.42	0.50	0.19	0.40
Hispanic	0.28	0.45	0.27	0.44	0.22	0.42	0.21	0.41	0.26	0.44
Parents completed high school	0.31	0.46	0.28	0.45	0.31	0.46	0.34	0.48	0.27	0.45
Parents have a college degree or higher	0.14	0.35	0.22	0.42	0.18	0.39	0.14	0.35	0.24	0.43
Single mother family	0.28	0.45	0.20	0.40	0.25	0.43	0.23	0.42	0.18	0.39
Bottom income category	0.15	0.36	0.08	0.28	0.11	0.31	0.09	0.29	0.06	0.24
Top income category	0.04	0.20	0.06	0.23	0.04	0.20	0.05	0.22	0.06	0.23
Attends urban school	0.59	0.49	0.43	0.49	0.55	0.50	0.59	0.49	0.43	0.50
Attends public school	1.00	0.00	0.98	0.13	1.00	0.00	1.00	0.00	0.98	0.14
Lives in the south	0.69	0.46	0.56	0.50	0.69	0.46	0.70	0.46	0.51	0.50
School hosts JROTC in 1980	0.98	0.13	0.92	0.26	0.96	0.20	0.99	0.10	0.92	0.27
School hosts a JROTC in 1982	0.98	0.13	0.93	0.25	0.99	0.08	0.99	0.10	0.92	0.28
Test scores, 10^{th} grade	16.77	15.01	22.93	15.76	17.40	15.40	17.96	15.83	25.24	15.64
Test scores, 12^{th} grade	20.57	15.59	27.96	17.65	21.45	17.04	22.61	16.42	30.19	17.47
Disciplinary problems, 10^{th} grade	0.42	0.49	0.27	0.44	0.37	0.48	0.39	0.49	0.20	0.40
Disciplinary problems, 12^{th} grade	0.25	0.44	0.17	0.37	0.32	0.47	0.27	0.45	0.20	0.40
Drop out between 10^{th} and 12^{th} grade	0.33	0.47	0.22	0.41	0.00	0.00	0.00	0.00	0.00	0.00
High school diploma	0.64	0.48	0.77	0.42	0.89	0.32	0.90	0.30	0.95	0.22
Postsecondary enrollment	0.38	0.49	0.58	0.49	0.44	0.50	0.45	0.50	0.67	0.47
Enlist in the military	0.23	0.42	0.07	0.25	0.24	0.43	0.26	0.44	0.08	0.27
Sample size	236		1,949		148		100		1,594	

TABLE A2 Detailed Bivariate Probit Results for High School Graduation, Early Participants

	Equation (1)	*Equation (2)*
	P(graduate=1| JROTC, X, S)	*P(JROTC=1| X, S, SJROTC)*
Early JROTC participant (9th or 10th grade)	0.432	—
	(0.246)*	
School hosts JROTC in 1980	—	0.724
		(0.197)***
School hosts JROTC in 1982	—	0.296
		(0.200)
Test scores in 10th grade	0.026	–0.010
	(0.001)***	(0.003)***
Female	0.112	–0.333
	(0.035)***	(0.070)***
Black	0.427	0.267
	(0.060)***	(0.096)***
Hispanic	0.301	0.070
	(0.048)***	(0.093)
Age	–0.349	0.095
	(0.027)***	(0.049)*
Parents, less than high school	–0.114	0.066
	(0.078)	(0.156)
Parents, some college education	0.134	–0.034
	(0.043)***	(0.086)
Parents, college degree or more	0.383	–0.023
	(0.070)***	(0.128)
Single mother	–0.339	0.092
	(0.048)***	(0.091)
Single father	–0.452	–0.083
	(0.097)***	(0.232)
Other family structure	–0.392	0.236
	(0.045)***	(0.088)***
Urban school	–0.153	0.316
	(0.053)***	(0.102)***
Suburban school	–0.041	0.068
	(0.042)	(0.097)
Catholic school	0.730	–0.129
	(0.073)***	(0.150)
Percent disadvantaged students in school	–0.002	–0.001
	(0.001)**	(0.002)
Percent faculty with graduate degrees	0.000	–0.001
	(0.001)	(0.002)
Number of federal programs offered by school (1980)	0.002	–0.023
	(0.007)	(0.014)
Number of federal programs offered by school (1982)	0.010	0.026
	(0.007)	(0.013)**
Observations	9,679	9,679
ρ	–0.32	
Chi-square on H_0: $\rho = 0$	6.33	
p-value	0.01	

Notes: The table presents the coefficients from a bivariate probit estimation. Other variables included in the model are: other race, family education unknown, family income dummies including one for missing income information, private non-Catholic school, and three region dummies. The control group includes non-participants who are white males, whose parents have a high school education, who live in a two-parent family household, are in the lowest family income group, and who attend a public school in the south. ***significant at the 1% level; **significant at the 5% level; *significant at the 10% level.